tends to have a predictable effect on smoking cessation. However, from a social perspective, it is fairly certain (to within relatively narrow confidence limits) how many cases of lung cancer or cardiac events will be avoided if 10 000 persons stop smoking. What this illustrates is that social benefit is more certain than individual benefit. In other words, if we are concerned with a social perspective, it is more consistent (when we are making health decisions on a social level) to use a discount rate which is lower than the simple aggregate of individual discount rates.

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1 West R. Discounting the future: influence of the economic model. *J Epidemiol Community Health* 1996; 50: 239–44.

Reply

I welcome the opportunity to explain some of the "misapprehensions" and "misunderstandings" detailed by Dr Marshall.

1. Economics for some may be the "science of choice" but the *Oxford English Dictionary* is a little more specific and describes it as the, "practical and theoretical science of production and distribution of wealth".

2. I had not intended in the introductory paragraph to claim that economic theory applied only to paid employment. I simply cited an observation in a widely used economic text[1] that the history of economics is pretty much the history of paid employment.

3. In Dr Marshall's third point (rewritten and expanded since I wrote my original reply) there seems to be a misunderstanding about the role of theory in science. In the cycle of "systematic enquiry, conjecture, and refutation", do we not accept the theory that best fits (explains) the observations, question the theory that fails "to fit all observations", and reject the theory when a new one seems to fit the observations better?—and thus did exit the "flat earth society".

4. Economics has certainly contributed to our understanding of community behaviour within the environment of paid employment. The suggestion was that other social sciences might contribute more (before or) outwith the environment of paid employment.

5. I understand that the theoretical basis of neoclassical economics is the individual—and in my naive view that may be the origin of the limitations of the neoclassical theory, because it overdoes the extrapolation from the individual to the community (the *Oxford English Dictionary* describes "economy" as the administration of resources of a community). That is the substance of my thesis.

6. Dr Marshall's discussion seems to demonstrate the dominance of "money" and the "perfect market" in economic theory.

7. The investment illustration appears to show the importance for economics of "finance". Since this non-health care example has been introduced, may I suggest that it shows simply that the purchase of a large "good" like a house is spread over time: there is no need to "ignore" or "assume".

8. It is most certainly not my perception that people who do not produce are not valued. The inclusion of the "human capital" model and the decision rules of "rational economic man" was to demonstrate that, as a moderately "compassionate caring society", we do not restrict health care planning choices to middle aged men in gainful employment.[2]

9. I recognised the possibilities of estimating implicit social value in health care decision making some 20 years ago,[3] and the idea was very rapidly adopted in the early days of health economics. Before that cost-benefit analyses (mostly in the field of transport economics) tended to enter an empirical or arbitrary social value and then compare cost-benefit ratios, but it seemed to me to be more logical to turn it round and estimate an "unknown" from an established equilibrium or status quo.

In conclusion, the closing sentence of Dr Marshall's letter that "social benefit is more certain than individual benefit" seems to endorse my thesis that "society's future is more certain than an individual's future". This might suggest that Dr Marshall may be beginning to drop some of the trappings of neoclassical economics—sovereignty of the individual and an assumption that society's choice may be predicted by the individual's. Let us hope that other economists will follow and continue to question the wisdom of discounting the future.

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1 Samuelson P, Nordhaus W. *Economics*. McGraw Hill, New York 1985.
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Ionizing radiation and offspring sex ratio

SIR—Dickinson *et al*[1] report a high sex ratio (proportion male) of offspring born to men exposed to ionizing radiation at Sellafield. These authors were undecided in their interpretation of this. I should like to suggest that their data represent strong further evidence that ionizing radiation directly (but admittedly weakly) affects offspring sex ratio.

If ionizing radiation induced sex linked lethal mutations, one would expect irradiated fathers to produce an excess of sons, and irradiated mothers to produce an excess of daughters.[2] In accordance with this theorising, Cox[3] reviewed the literature and reported that in 12 data sets describing the offspring of irradiated fathers, there were 10 in which sex ratios were raised in contrast with those of controls: and in 10 sets describing the offspring of irradiated mothers, 9 reported lowered offspring sex ratios ($\chi^2 = 9$, $p<0.005$). So there is good prior evidence that ionizing radiation has the effects described. The data of Dickinson *et al*[1] conform with this generalisation. The sex ratio of offspring born to their most heavily irradiated men (201 sons, 144 daughters) is highly significantly different from that of the offspring of the women who had received preconceptual radiation (454 sons, 461 daughters) ($\chi^2 = 7.1$, $p<0.01$). So the data of Dickinson *et al*[1] add to the already existing strong evidence that ionizing radiation induces sex linked lethal mutations in man which are reflected in the offspring sex ratios of irradiated parents.

The effects of ionizing radiation are quite different from those of non–ionizing radiation in this regard. For instance, men working in high voltage power stations or substations reportedly produce an excess of daughters,[4–6] possibly as a consequence of hormonally mediated effects of such radiation. In general, illness and adverse industrial/occupational exposures to men are associated with low testosterone and/or high gonadotrophin levels, and with low offspring sex ratios (presumably as a consequence of the hormonal disturbance).[7] Thus—as far as I know—ionizing radiation is the only reproductive hazard which causes men to sire an excess of sons.

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1 Dickinson HO, Parker L, Binks K, Wakeford R, Smith J. The sex ratio of children in relation to paternal preconceptual radiation dose. *J Epidemiol Community Health* 1996;**50**:645–52.
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Reply

We thank Dr James for his letter and, in particular, for reminding us of the theory which Schull and Neel put forward in 1958 that irradiated fathers and mothers would be expected to produce excesses of sons and daughters respectively.[1] As knowledge of genetics progressed, Schull and Neel reconsidered this theory,[2–4] their most recent postulate being that while maternal exposure would, in principle, reduce the sex ratio, it is difficult to predict what effect irradiation of fathers might have since, "With the recognition that one X chromosome is inactive in the somatic cells of women (the Lyonization phenomenon), it became clear that sex linked mutations induced in males were unlikely to have a dominant lethal effect in females."[4]

James also brings to our attention the study by Cox, who claims that 10 out of 12 data sets describing the offspring of irradiated fathers showed an increased sex ratio in exposed groups.[5] All the studies of paternal irradiation to which Cox referred were summarised in table 4 of our paper,[6] where we gave the minimum detail necessary for critical assessment. Such a review must inevitably be dominated by Schull and Neel's study of 53 691 children born during 1948–55 to the survivors of the atomic bombs dropped on Hiroshima and Nagasaki.[1] Subsequent to Cox's review, Schull *et al* updated this work by further considering the 47 624 children born during 1956–62 to the Japanese atomic bomb survivors,[2] the relevant results again being summarised in table 4 of our study.[6] Many of the smaller studies appeared to classify children as "exposed" if they were born, rather than conceived, after paternal irradiation.[7–10] Several were questionnaire based studies[9–12] whose response rate ranged from 37%[9] to 64%.[11]

The studies of the atomic bomb survivors[1 2] considered children conceived at least 18

months after the bombings and thus excluded the effects of radiation doses received by fathers during the period of spermatogenesis—ie immediately before conception—whereas we reported an association with the dose estimated to have been received by fathers in the 90 days before conception. Referring again to table 4 of our paper,[6] it will be seen that only in the studies of chronic exposure is it likely that the father was irradiated in the immediate preconceptional period and that these studies yield equivocal results.

James' post-hoc comparison of the children born to mothers and fathers who had received a preconceptional radiation dose is invalid: our analysis considered an a priori hypothesis comparing both these groups independently with the remainder of the population, after allowing for the effect of year and paternal employment at Sellafield. Since the sex ratio of children of mothers with a preconceptional radiation dose was 0.985 (95% CI: 0.865, 1.121), not significantly different from that of children of mothers without such a dose, 1.046 (95% CI: 0.961, 1.140), our data provide no evidence to support the hypothesis that irradiated mothers are more likely to produce girls.

One of the caveats concerning our results which we would like to reiterate is our concern about the estimation from annual dose summaries of radiation doses received during the 90 days before conception—a process which inevitably leads to dose misclassification. While random misclassification generally biases the results towards the null hypothesis, in this instance it is quite possible that there is differential dose misclassification and hence uncertainty about the direction of bias. Measurement error is clearly an important issue in this regard and is an area we are actively exploring.

To summarise, studies of the possible association between parental preconceptional irradiation and an altered sex ratio do not yet satisfy the Bradford Hill criteria for inferring a causal relationship.[13]

We would like to take this opportunity to thank Mr Les Scott, Dosimetry Data Manager at Westlakes Research Institute, for estimating the preconceptional radiation doses on which the study was based.

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NOTICES

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BOOK REVIEWS

Lecture notes on epidemiology and public health medicine. 4th ed. Richard Farmer, David Miller, and Ross Lawrenson (Pp 288; £13.95) Oxford: Blackwell Science, 1996 ISBN 0-86542-611-2.

The fourth edition of this well established text bears little relation to the thin tome that I used at university in the early 80s. The book has been expanded and is well laid out with good illustrative diagrams and extensive use of bullet points. The latest edition has been excessively updated and includes a new chapter on health targets with a particular emphasis on the priority conditions identified in *The Health of the Nation*. Many of the latest changes in the Health Service are described, although the pace of change is such that the most recent mergers, of district health authorities and family health service authorities, have not been included.

In general the book is an excellent introduction to the subject and any criticisms are fairly minor. In the chapter on survey methods, it was sometimes not clear whether the term "bias" was being used in a general or technical sense with a definition only appearing towards the end of the chapter. The discussion of this important concept could have been expanded within one section rather than spread through subsequent chapters. In discussing Health Service structure, there is a heavy emphasis on describing the Health Service in England, the other countries in the United Kingdom meriting only one sentence. One diagram is erroneously labelled as illustrating the Health Service in the United Kingdom when, in fact, it only shows the structure in England. A further chapter giving some general international perspectives in health service organisation could also have been usefully added.

Notwithstanding these comments, the book should be a recommended initial text for medical students, and serve as useful summary of the subject for clinicians wishing to update their knowledge. Postgraduate students commencing study in public health might also find the book useful.

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Adult mortality in Latin America. Ian M Timaeus, Juan Chackiel, and Lado Ruzicka. (Pp 367; £45.00). Oxford: Clarendon Press, 1996. ISBN 0-19-828994-4

Latin America, as other world regions, is changing its demographic and epidemiological pictures. The adult population is growing and it is essential to analyse health conditions of this group. This volume is focused on the mortality of that segment of the population. The book originated from the First Scientific Seminar of the Adult Mortality Committee held in Chile in 1991. In spite of the old information on which the papers are based, the diversity of subjects, and diverse author approaches, it is a good summary of the burdens and contrasting situations characterising the changes in this region.

The book has four parts. The first presents a comprehensive overview of the book content and an expert's paper with a complete description of trends and demographic characteristics of Latin America. The four papers comprising the second part concentrate on the different methods of studying adult mortality and on the limitations of data sources in underdeveloped environments. Part three concerns itself with current changes in the epidemiological patterns in selected countries. The corresponding analysis is a reminder of the complexity of the transition phenomena in countries with vulnerable economies and increasing social conflicts. The negative effects of global crisis on public health programmes and the big contrasts within the countries, are important considerations made by the authors. The last section is the longest and is concerned with specific causes of death and their prevention.

The quality of the papers is variable—it is worth mentioning, however, the remarkable paper on maternal mortality by Rajs. The reduction in cardiovascular mortality seen in some countries is difficult to explain because of the scarcity of effective preventive intervention and health promotion programmes. The excessive deaths due to all kinds of violence are striking, even more when it is considered that violence ranks first in morbidity and mortality in several countries. Some of the conclusions arrived at by the authors in this section are fascinating and challenging, particularly those by Frejka and Atkin on abortion.

This book is a valuable contribution to the subject of adult health in Latin America. However, several questions are still unanswered. 1.